DOCUMENT RESUME

ED 109 719 CS 501 089

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TITLE A Critique of Theoretical Contributions in Instructional Communication.
PUB DATE 75
NOTE 14p.; Study prepared at West Virginia University; Not available in hard copy due to marginal legibility of original document

EDRS PRICE MF-$0.76 PLUS POSTAGE. HC Not Available from EDRS.
DESCRIPTORS *Analytical Criticism; Class Activities; *Classroom Games; *Communication (Thought Transfer); Creative Dramatics; Higher Education; *National Surveys; *Simulation; Teaching Methods

ABSTRACT

In "Theoretical Contributions in Instructional Communication," a survey was presented concerning the extent to which communication games are being used in communication classrooms. The survey sampled 293 Speech Communication Association members. An unequivocal or representative answer to the question, "Which types of games are utilized?" was impossible for two reasons. The authors arbitrarily limited the number and types of games available to the respondent on the questionnaire, and the games described for the respondents were so "semantically" rich that they may very well have elicited differential meaning from the respondents. However, the authors should be applauded for attempting the study, and a duplication of their effort is warranted. (RB)

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ED109719

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EDUCATION & WELFARE
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A CRITIQUE OF THEORETICAL CONTRIBUTIONS IN INSTRUCTIONAL COMMUNICATION

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A CRITIQUE OF THEORETICAL CONTRIBUTIONS IN INSTRUCTIONAL COMMUNICATION

God only knows, I've spent a good many sleepless nights wondering about what parents might say if they entered a basic communication course and discovered their angelic daughter groping with some lusting undergraduate male in the "Between the Sheets" exercise. By the same token, the same person on high knows that I've tossed and turned a good many nights as I've tried to imagine what parents might say if they entered the same course and found their blue-eyed male child who had never been closer to a black than the pages of an old issue of National Geographic, shouting obscenities at some fictitious slumlord as he convincingly role-played the first-born of Huey Newton.

While they might say "and for this we pay 1500 big ones a year," I think I can conclude with some degree of certainty that one wouldn't turn to the other and say, "Aha--you see, dear, Professor Smith employs the creative, experiential, conceptualization and concept formation through inductive reasoning with existential implications method of teaching communication." The point I'm trying to make here is about as subtle as Richard Nixon's offer of the Directorship of the F.B.I. to Federal Judge Matt Byrne while he was presiding over the trial of Daniel Ellsberg. Higher education is currently being attacked on the grounds that it has never been accountable to the 190 million people or so who support it. And the attackers have made it perfectly crystal clear that those of us involved with higher education had better make ourselves accountable if we are to expect the monetary and attitudinal support that we have enjoyed in the past to continue.

Now, in my mind, the issue of accountability holds important implications for those of us in the business of human communication. Because we traditionally have been vulnerable to this kind of attack. And lately, I've come to the conclusion that we are presently and particularly vulnerable in terms of our classroom pedagogy.

So, it was with some measure of excitation that I approached the task of

reading and evaluating Professors Cheatham and Erickson's survey concerning the extent to which "so called" communication games are being used in our communication classrooms.

To begin with, then, I would like to congratulate Professors Cheatham and Erickson for taking on an issue that needed to be confronted. While they do not come right out and say it, I was left with the impression that they were both concerned with whether or not the numerous communication games and simulations that are being used in our classrooms are pedagogically justified. I share and applaud that concern.

Toward that end, the authors surveyed a sample of the Speech Communication Association membership. The survey itself was designed to elicit member responses to a number of questions concerning the use and justification of communication games in the classroom. I think, for the most part, these questions were conceptually sound. But I have some reservations about whether the survey was designed in such a way that unequivocal answer or answers representative of communication educators to the most significant questions could realistically be obtained. For example:

Which type of games are utilized? In my mind, an unequivocal or representative answer to this question was impossible for two reasons. First, the authors arbitrarily (I think) limited the number and types of games available to the respondent. How are we to know, then, whether the response is a true indicant of the most widely utilized games or an indicant of the most widely utilized games among those available to the respondent?

The answer, quite simply, is that we do not know because we can't be certain that the five types of games available to respondents constitute a representative sample of the universe of games that are presently available. Second, the types of games available to the respondnet were semantically rich. That is, I think they may very well have elicited differential meaning from the respondents. To infer, consequently, that the 293 respondents indicating they used reality simulation

held identical meaning for the comcept is unwarranted, and, therefore, the responses are uninterpretable.

Why do instructors use communication games? The same criticisms are applicable to the responses to this question. Again, the authors artitrarily limited the number of rationale factors available to respondents. Upon what basis? Again, there is no guarantee that these rationale factors elicited identical or even similar meaning in the minds of the respondents. Their own data, in fact, support this concludion. The responses to the open-ended questionnaire item, for example, do not necessarily "echo" the most frequent response to the question of why (because they are a superior method of clarifying); although the authors have interpreted them as such. How am I or anyone else who reads the paper to know whether the authors' interpretation of one subject's response that "an ounce of experience is worth a ton of talk" or another's that "learning to do by doing" is accurate in this regard? In short, two responses that possibly "echo" the most frequently chosen rationale factor do not warrant the conclusion that is drawn (see pg. 4).

How do instructors evaluate students' participation in communication games? While the preceding criticisms also apply here, I would like to say something about the functional utility of the responses to this question. Given the nature of the questionnaire item and the limitations in the types of responses that could be elicited, the authors were more or less forced to conclude little beyond the fact that all types of evaluation methodologies are used; either by themselves or in conjunction with others. While I don't mean to sound denigrating, I can't help but wonder whether the responses to this question are pedagogically useful. I think they could have been — that is, if the questionnaire item had been designed in such a way that it would have enabled the authors to conclude something about the specific type and form of evaluation whether it be peer, instructor, pencil-paper, or some combination of evaluative techniques. Also, this kind of information, when coupled with the information which the authors did receive, might have been

useful for comparative purposes.

Which communication concepts are currently being taught through communication games? The question of meaning is particularly significant to this questionnaire item. For example, did the authors define in the questionnaire what they meant by "concept?" If they didn't then how can they be sure that the 68 respondents indicating nonverbal communication or any other topic area had the same concept in mind? For example, while the concept nonverbal communication could mean communicating without verbal symbols in general, it could also mean communication through the use of body, space, time, or touch. But again, we really have no reliable way of knowing this.

While I think that these criticisms also speak to the question regarding criteria for selecting a particular game, I would like to move on to some methodological issues.

Sample. I question whether the sample utilized in this study was representative of the membership of the SCA. What do we know, for example, about the population from which the sample was drawn? Probably very little beyond the fact that it is highly heterogenous -- made up of a number of divisions with divergent interests, methods, and pedagogical goals. Since we do not know whether there are differences between these divisions as a function of teaching style or other relevant variables, it is inappropriate to collapse across these divisions to derive one population. Yet, that is exactly what occurred in this survey. The membership of the SCA was treated as one population when it may very well be that the SCA is made up of many different populations. As a result, a stratified sample may have been more appropriate than a random sample.

Construction of the questionnaire items. Much of the criticism in the first part of this critique is predicated on the assumption that the questionnaire items used in this survey were intuited rather than derived in some systematic fashion (e.g., independent panel of experts). If that is in fact the case, then surveyor bias severely mitigates against the representativeness of the responses to the

questionnaire.

The items themselves. I can sympathize with the difficulties two researchers automatically impose upon themselves when they decide to conduct a large scale survey. As a result, I can understand why the authors strived to make the rask of completing the questionnaire as easy as possible. However, when they assume this kind of research posture, they also run the risk of compromising the representativeness of their survey results. For example, I am somewhat puzzled by their decision to employ items that were limited to categorical replies as well as their failure to ask respondents to rank-order their replies where appropriate (e.g., questions 2, 4, 5, 6, and 8). I think the meaningfulness of the survey results could have been significantly enhanced if the questionnaire items had been constructed in such a way that interval data could have been obtained or, if this were impossible (I don't think it is), to have respondents rank-order their replies.

Attitude Scales. Finally, one item attitude scales such as those employed in this study are notoriously unreliable (Kerlinger, 1964). As a result, the responses to these scales may not be representative of teacher, student, or colleague attitudes concerning communication games. That is unfortunate because represnetative findings in this regard would have been very useful.

In sum, I think the authors started out with some good questions that needed to be answered, but failed to design their survey in such a way that they could obtain representative answers to the questions. However, in my mind, that is what research is all about. We learn as a function of both our successful studies and our not so successful studies. And, in my case, the latter is much more true than the former.

In closing, I hope you find at least part of this constructive. It has not been my intention to attack but to probe and I hope the preceding criticism is so perceived. Anyway, I think a duplication of their effort is warranted and that they should be applauded for attempting to study something that is very difficult to nail down.

I think Ms. Haynes' paper concerning the implementation of a mastery learning system of instruction also has something to say about the state of our communication classrooms. On the whole, there is very little to quarrel about with respect to the intentions of this paper. Mastery learning is indeed a desirable instructional approach -- one that is compatible with both the goals of teachers and the current holders of the purse strings hiding behind the banner of accountability. And, I agree with Ms. Haynes' conclusion that time constraints imposed by the academic calender do not or should not preclude the possibility of implementing mastery learning in any classroom.

My criticism of the paper, consequently, is not so much concerned with what Ms. Haynes did advocate as with what she did not advocate. But let me explain. In the beginning of this paper, I think some unwarranted conclusions are drawn -- conclusions that in my mind might prove misleading for the naive reader. And I think they set the tone of the entire paper. For example, consider the following conclusions taken from the first page of Ms. Haynes' paper:

> "In actuality, there are two ways to set up a mastery learning system." To paraphrase, one in which the students are allowed unlimited time to acquire certain skills and one in which students, depending on their individual skills, attempt to acquire varying skills within a constant amount of time.
>
> "Varying the number of skills to be acquired is particularly appropriate to classes such as public speaking, <u>where teachers recognize the difference in students' capabilities and do not expect all to reach the same degree of proficiency</u>."

A number of things need to be said regarding the preceding statements. First, they are highly misleading if not incorrect. As a case in point, there are several modifications of mastery learning systems -- any of which can be effective if systematically developed. But that is not nearly as troublesome as the second conclusion in which Ms. Haynes asserts that public speaking teachers somehow or the other are better equipped than other kinds of teachers to assess student abilities and are perhaps more sensitive to the fact that students shouldn't be expected to achieve at the same level. Aside from the fact that this conclusion seems to be

inconsistent with one goal of mastery learning -- i.e., having all students meet a high criterion level -- it is contrary to what one would be led to believe on the basis of research. Experiments concerned with the so-called "expectancy effect," for example, suggest that teachers are more than capable of erroneously judging a student's ability and that these erroneous judgments often impact on a student's achievement throughout a course (Rosenthal and Jacobson, 1968; Meichenbaum, Bowers, and Ross, 1969; Rubovits and Maher, 1971). If that were not enough, there is considerable evidence in support of the notion that teachers often mistake physical or psychological disabilities that may impair achievement for something they are not (Silverman, 1968). Admittedly, this research did not focus on speech teachers specifically. But why should I or any other reader of this paper conclude that public speaking teachers are exempt from making bad judgments?

What I am attempting to say here is that the preceding conclusions misrepresent the facts. And I think this kind of misrepresentation occurs throughout the paper. Why is that? Well, in answer to my own question, I think Ms. Haynes made a very conscious attempt to explain mastery learning and how it might be implemented in such a way that the naive reader would not be bowled over by the jargonese of the instructional technologists. That is to be applauded. But in the process, I think she over simplified both mastery learning and the task of implementing mastery learning in any classroom -- regardless of the subject matter being taught. Thus, I am somewhat afraid that a naive teacher might read this paper, conclude that he or she has an adequate understanding of mastery learning, and decide to implement a mastery learning system in accordance with the dictates of the paper. But the truth of the matter is that he or she wouldn't have an adequate understanding because the paper fails to address some crucial issues concerning mastery learning that are inherent to its understanding.

Let's begin with the issue of why mastery learning is superior to other pedagogical approaches. In my mind, the advantage of any mastery learning program is that it requires students to master a specified number of behaviors judged to be

essential to the subject at hand. By master, however, I am not talking about students achieving a "C" or "C-", which Ms. Haynes, I assume, thinks is proof enough of mastery. Instead, I am talking about students meeting some specified criterion, usually somewhere in the neighborhood of 80 per cent of the specified attributes of the behavior. And that, quite frankly, is why mastery learning can be a superior pedagogical approach. Its purpose is not simply to eliminate failure, but to maximize successes and a "C" or "C-" is not an indicant of success. But this brings up yet another issue that Ms. Haynes failed to address or warn her readers about.

A mastery learning system is only as good as the criterion-referenced system of grading that accompanies it. And good criterion-referenced systems of grading are about as easy to come up with as truly non-sexist males. And this is true in hard science subject matters much less public speaking courses. In short, I think it was her duty to warn the readers that if they couldn't establish a reliable and valid criterion-referenced grading system that it would be a waste of both their and their students' time to attempt to formulate and implement a mastery learning system of instruction.

The issue of grading is further confounded by the way in which a teacher decides which behaviors are essential to the development of his or her students. Needless to say, I don't think that a teacher should rely solely on experience when it comes down to deciding which behaviors all students must master. What this suggests, then, is that a teacher approach this task in some systematic fashion -- perhaps an audit or taxonomy of essential behaviors. If they do not, again something Ms. Haynes failed to warn about, there is no guarantee that the behaviors they require students to master are worth mastering.

Finally, I think teachers would be doing their students a tremendous disservice if they attempted to intuit "the strengths and needs of students, identifying students for academic groupings and to serve as leaders of groups." Teachers

have about the same reliability in this regard as one-item measurement scales. As a result, I think teachers would be better advised to seek out existing diagnostic measures of learning variables, predispositions toward varying instructional methods and content, and certainly levels of apprehension associated with oral communication. I make mention of the latter because my own research suggests that learner history may significantly effect conative and affective behaviors in the type of instructional system that you advocate. By assessing them prior to entering the instructional system, a teacher is placed in the advantageous position of being able to control them.

In sum, I do not disagree with what is said in the paper. I object, quite simply, to what is not said in the paper -- and that could easily be rectified.

Before turning to the Todd et. al. paper, I would like to say something about Bill Todd. I first met Bill some seven years ago while we were both undergraduates at California State University at Long Beach. Unlike me, Bill was extremely conscientious as an undergraduate -- so much so, in fact, that Tom Young and I had to drag him by the hair after classes to partake of the hops. After receiving this paper, which was difficult to carry muchless critique, I am convinced that he hasn't changed.

My comments about this review of the diffusion literature are based on the following criteria:

1. Was the review comprehensive?
2. Was the review well written, organized, and documented?
3. Was the review critical in nature or was it a simple summary of research?
4. Was the review systematic; that is, in the sense that it organized research in such a way that it might suggest postulates or principles, laws, or theory?
5. Was the review heuristic?

Criterion 1. I think I can safely say that this review is as comprehensive,

if not more so, than any review it has been my pleasure to read. I think the author are to be commended, moreover, for attempting to integrate literature that in the past has only been tangetially related to research concerning the diffusion of innovations, with literature reviewed in extant sources such as Rogers and Shoemaker

Criterion 2. Given the unusual format of this review, I think the authors did an admirable job of writing. It is by no means an easy task to synthesize literatur and present it in a comprehensible manner -- something I think a good many people in this discipline overlook. By the same token, the task of abstracting a study, think-piece, or review so that they remain semantically intact is far from being a push-over. And to a large extent, I think that this review was more than satisfactory with respect to both tasks.

While the organization of the paper set well with me, I think it could conceivably get in the way of the content for some readers. Why is that? Well, a review of the literature organized such as this is far from typical. And I am afraid that some readers might be turned-off to it because they are unaccustomed to processing information as it appears in this paper. Speculative as that initially may seem, I think it is tenable. As a result, the authors might want to subject portions of the content to measures of readability and/or satisfaction. My own research, though still in the exploratory stages, suggests that various methods of presenting content elicits differential levels of satisfaction and that there may be some causal linkage between satisfaction and comprehension.

Needless to say, the paper is well documented.

Criterion 3. In my mind, there are at least two ways to look at the question of what constitutes a critical review of the literature. The perspective that I have generally subscribed to is rather straightforward: a critical review of the literature is one in which the works that are reviewed are conceptually and operationally critiqued. In effect, this means that the reviewer makes editorial comments about the thesis or rationale of a particular work, whether statements concerning possible relationships are suggested as a function of the thesis or

rationale, whether appropriate methodologies are employed in the effort to discern the nature of these relationships, and finally, whether the preceding have been done in such a way that the claims (knowledge or otherwise) made in the work are justified. In my estimation, this review of the literature is not critical in this sense.

But as I said before, there is at least one other type of critical review. This second perspective seems to be predicated on the assumption that if one decides to review only that which is appropriate to the needs at hand, he or she has engaged in a critical process. I think this paper conforms to this description much more than the preceding one -- and, I think this is a possible limitation in it.

Criterion 4 and Criterion 5. I've decided to collapse these two because I think that if you have met one, then the conclusion that you have met the other is inescapable.

Now, I would like to say that this review is anything but systematic. By doing so, I might be able to shield myself from the accusation that this critique is inocuous. But alas, I can't. The paper is not only systematic in thrust, it is systematic by design. And I think that this is perhaps the true strength of the paper. The authors have formulated a model based upon weaknesses of existing models, perused the literature in the attempt to support the model, and kept intact or revised certain aspects of the model as a function of the perusal.

Finally, I think the review is heuristic in the sense that the supporting literature suggests causal linkages between variables in the diffusion process. At the same time, however, I do not think that these causal linkages or, perhaps more appropriately, the relationships between the variables explicated throughout the various stages of the model were made explicit enough. If they had been, I think the review and the model could have been significantly extended -- so much so, in fact, that principles or propositions concerning the diffusion of educational

innovations would not be left to the deductive prowess of the reader. In short, I think the model and review in support of it have real theoretic implications; that is, if the authors take full advantage of the implicit relationships the model and review currently suggest. If they do not, moreover, I think they run the risk of eliciting a "so what" response from many of those who might have the most to gain from reading this paper.

As a final note, I would like to say something about the papers on this panel in toto. In my mind, each makes an honest attempt to explain, modify, or extend the parameters of human communication instruction. And since I think that human communication theory's real import is in the classroom, I find these attempts not only admirable, but worthy of modeling.